

Book Review

Radical Behaviorism and Behavior Analysis: A Review of *Behaviour Analysis and Contemporary Psychology*

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This book contains papers originally presented at the First European Meeting on the Experimental Analysis of Behavior held in Liege, Belgium in 1983. The title of the book, *Behaviour Analysis and Contemporary Psychology*, which was also the title of the meeting, reflects the integrative spirit recommended by Richelle in the first chapter. The editors selected the papers from conference presentations in areas of research where dialogue with other traditions seemed desirable and fruitful. Those areas were ethology, psychopharmacology, and human behavior. The category of human behavior included papers on rule-governed behavior, the concept of operant behavior, the matching law, the relation between basic and applied research, neurophysiological rehabilitation, autism, and verbal behavior. The book includes a chapter by Skinner entitled "The Evolution of Behavior," and it ends with a review of contemporary behaviorism by Blackman.

Every book reviewer begins with presuppositions about what is important and worthwhile, and I am no exception. It seems best to state my central presupposition from the outset and to acknowledge that it has affected my reading of the book. I am committed to the insistence that, at the core of radical behaviorism, behavior (read, action) is both a subject in its own right and the proper subject matter of psychology. In reading

this book, I was interested primarily in how its contents might contribute to a psychology of action, as opposed, of course, to the psychology of organocentric processes and ideal types pursued by the rest of psychology. I have reviewed the book in that light.

BEHAVIORISM

Should we expect a commitment to radical behaviorism in a book on behavior analysis? As Hayes (1978) has noted, behavior analysts evince varying degrees of commitment to radical behaviorism and behavior analysis. Our commitment is technical when we use shaping, fading, and other such techniques; methodological, when we use intrasubject replication designs; conceptual, when we use the concepts and principles of behavior analysis; and philosophic, when we are committed to radical behaviorism. Some of us have a technical involvement in behavior analysis, others a technical and methodological involvement, and so on, with a full commitment including an involvement in the philosophy (Hayes, 1978). The less-than-full commitment of many of us is apparent in the conceptual poverty of applied behavior analysis, in the infiltration of cognitive terminology, in the re-appearance of traditional group designs, and in the reduced interest in behavioral control techniques, among other things (e.g., Cullen, 1981; Ferster, 1978; Hayes, 1978). Richelle (Chapter 1) implicitly acknowledged these departures when he noted that the seminal views presented by Skinner have not led to the conceptual and experimental work we might have expected.

In light of these realities, looking for contributions to radical behaviorism in

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a book on behavior analysis is probably unrealistic and perhaps unfair. Without a commitment to radical behaviorism, however, behavior analysis is increasingly difficult to distinguish from the rest of psychology. After all, the full commitment offers an ongoing and radical revision of traditional preconceptions about psychology's subject matter, tasks, and methods. Is the basis for Richelle's integrative spirit that behavior analysis is no longer distinctive from the rest of the psychology? I prefer to think that talk of integration means that behavior analysis, informed by radical behaviorism, has something systematic to offer the rest of psychology and that we want to open the channels of communication. I will persist, then, in discussing this book for its contribution to radical behaviorism.

Is radical behaviorism mentioned in the book? Both Richelle (Chapter 1) and Blackman (Chapter 16) mention it explicitly. Richelle noted that people who pursue the experimental analysis of behavior share the methodological and epistemological position known as radical behaviorism. He also noted, however, that behavior analysts show varying degrees of commitment to radical behaviorism. Likewise, Blackman's comments reflected some equivocation about how radical behaviorism relates to behavior analysis. Blackman said that behavior analysis provided an experimental foundation for radical behaviorism. But, he also noted that developments in behavioral pharmacology suggest we can separate experimental work in behavior analysis from philosophic work in radical behaviorism. These comments reflect a collective uncertainty about how radical behaviorism relates to behavior analysis.

Given this uncertainty, how did the contributors present radical behaviorism? Richelle offered some remarks pertinent to this question, but I want to comment on Blackman's characterization of radical behaviorism. He noted that radical behaviorism departs from traditional psychologies, which conceptualize behavior as a manifestation of central processes. Radical behaviorism rejects that tradition, in particular, by taking action

as a subject matter in its own right. Blackman also pointed out that radical behaviorists explain behavior in terms of environmental variables. This latter assertion, and the characterization of radical behaviorism it implies, commonly appears in our literature. My increasing discomfort with this ascription of environmentalism has much to do with my dissatisfaction with some chapters in this book. That dissatisfaction arises because I see our enterprise as a contingency-oriented psychology, something that ascriptions of environmentalism too easily conceal.

We tell (or should tell) our students that reinforcement and punishment, our basic functional relations, are about contingencies and the effects of contingencies on performance. Contingencies, however, are not environmental variables in the sense of being outside the skin of the organism. Some contingencies are transdermal variables—variables that stretch across organism and environment (e.g., a food pellet follows if and only if the rat presses the lever). Other contingencies are intradermal variables—variables located organismically and not environmentally (e.g., changes in the configuration of the finger nails contingent on nail biting). We identify contingencies as important variables in our experimental and interpretative work, and claim that they explain behavior. But if contingencies are not environmental variables, what becomes of the claim that we explain behavior in terms of environmental variables? Also, what becomes of expressions such as "environmental contingencies," "outer contingencies," and the like? It seems that what we say lags behind what we do. That is, we are committed verbally to the environment, but experimentally (e.g., Sidman, 1986a, 1986b), descriptively (e.g., Glenn, 1983; Goldiamond, 1984), and conceptually (e.g., Day, 1983; Sidman, 1986a, 1986b) to contingencies.

All of this brings me back to radical behaviorism and its relationship to behavior analysis. We might clarify this relationship if we could abandon the term "radical behaviorism." It misleads, it alienates, and it blocks communication.

Who wants to be labelled a behaviorist these days and particularly a radical one? Even Skinner (in Evans, 1968, p. 24) suggested that the term "behaviorism" is a misnomer in the context of radical behaviorism. I prefer Pierce and Epling's (1980) characterization of our enterprise as a contingency-oriented psychology. This characterization spells out the essence of radical behaviorism and gives our enterprise a name that states explicitly what we are about. If a contingency-oriented psychology is what we are about, then the philosophic and interpretative aspects of our work (radical behaviorism) and the empirical aspects (behavior analysis) *must* be related, at least to whatever extent they contribute to a contingency-oriented psychology. (By the way, one author overlooked the distinction between methodological and radical behaviorism when he spoke of methodological and radical behaviorism in one breath. This mistake seems inexcusable now, particularly given Day's (1983) excellent account of the distinction.)

BEHAVIOR

I was curious to see how the contributors dealt with the concept of behavior, which, after all, underlies the assertion that behavior is the subject matter of psychology. I will mention the comments of three contributors.

Le Ny (Chapter 2) noted the ambiguities of "behavior" and traced something of its etymology. The ambiguities of "behavior," across activity, movement, and action or conduct, have been widely noted (e.g., Coulter, 1982; Hamlyn, 1953; Kitchener, 1977; Purton, 1978), but it was interesting to read a European perspective on the issue. Le Ny also discussed Pierre Janet's concept of conduct which admits large units (e.g., giving a lecture) and does not sharply distinguish private from public events. Le Ny saw this latter feature of the concept as a disadvantage. But, given comments elsewhere about the public-private continuum (e.g., Hineline, 1980), Janet's concept of conduct seems close to what we mean by "behavior," in this respect

at least. Anyone wanting to build on Kitchener's (1977) account of "behavior" might profitably review Janet's concept of conduct.

Colpaert (Chapter 14) reminded us that behavior emerges from the nervous system and is sensitive to multifarious conditions. He must have meant behavior in the sense of bodily behavior, four-dimensional morphology, or organismic activity. In this sense, there can be no new behavior, at least not without a new species. Our subject matter is grounded in the biological organism and its activities, but psychology is not about behavior in the sense of bodily activity. For a start, behavior of psychological interest is behavior classified by its end-results. These functional classes are classes of bodily behavior, albeit classes that depend neither on any particular body segment nor on any particular movement (Hunter, 1932). Hidden in the concept of a functional class is the realization both that bodily behavior is significant psychologically because it participates in contingencies and that contingencies, not behavior as such, constitute the proper interest of psychology. Contingencies are our fundamental units (e.g., Day, 1983; Sidman, 1986a, 1986b; Weingarten & Mechner, 1966), and they are tantamount to the actions that Murray (1938) wrote about. Contingencies (or actions in Murray's sense) give us a subject matter grounded in bodily behavior but constituting something more than bodily behavior.

Delius (Chapter 5) argued that to understand any behavior, we need contributions from many disciplines, including behavior analysis, ethology, genetics, economics, and sociology. Delius said "Everybody should join the melee!" But melee it has been and will continue to be. I was disappointed to see a contributor to a book with this title advocating the antithesis of radical behaviorism. But, the grounds for my disappointment need some explanation.

The argument that behavior requires multidisciplinary investigation is not new (e.g., Blurton-Jones, 1976; Fordyce, 1971; Manicas & Secord, 1983; Parmelee,

1924). For instance, Parmelee (1924) argued that the science of behavior is a hybrid of biology, psychology, and sociology, and Blurton-Jones (1976) commented that we need information from biochemistry through sociology to explain even the simplest aspects of human development. No one would deny that behavior, *understood as the activity of an organism*, is a multi-level phenomenon requiring information from many disciplines. Agricultural phenomena are the same, as, doubtless, are all natural phenomena.

We can accept that phenomena outside the laboratory are multi-levelled and yet still insist that psychology is an autonomous science with its own level of integration. Other experimental sciences abstract out particular levels and study them to the exclusion of other levels. For example, physics studies the physical level of integration and ignores chemical and biological involvement for its own purposes (Whitehead, 1953). Insisting that behavior (read, action) is a subject matter in its own right recognizes that psychology properly deals with action as a subject matter *and* as a level of integration abstracted from multi-level reality (see also Woodger, 1956, pp. 117–118).

Psychology has neglected conduct and has tried to deal with human beings and nonhuman animals in some comprehensive and ill-defined sense. Critics have long discussed the resulting absence of peculiarly-psychological facts and the logical impossibility of conceptual systematicity in a discipline of such breadth (e.g., Hunter, 1932; MacLeod, 1965; Peters, 1953, pp. 23–24; Roback, 1923, p. 24). To a radical behaviorist, psychological facts and conceptual systematicity are found in a domain comprising contingencies, the functional relations that contingencies enter into with behavior, and the other functional relations that emerge (Sidman, 1986a, 1986b). This domain does not constitute the whole of behavior (read, “organismic activities”) but, rather, only one level of it. This abstracting out of contingency phenomena is a limitation but not a disadvantage. Indeed, in light of Whitehead’s (1953, pp. 249–250) discussion, it is an advantage.

In summary, I was disappointed to find free-for-all research (Deliuss’ term) advocated in a book with this title, particularly when radical behaviorism departs from this traditional aspiration. Certainly, if we want to find out all there is to know about the pecking of a pigeon (Deliuss) or the reading of a child, then we need information from many disciplines. But a contingency-oriented psychology is only one of those disciplines and will make its special contribution by concentrating on its particular level of integration to the exclusion of all others.

INTERESTING DIRECTIONS

I have expressed my disappointment with aspects of the book on the grounds that some contributors did not pursue a contingency-oriented psychology. I now want to mention several chapters that did contribute to a contingency-oriented psychology.

Verbal Behavior

The chapters by Lowe and Horne (Chapter 7) and by Catania (Chapter 9) were relevant to verbal behavior. Catania related the origin of verbal behavior in the human species to instructional control. Lowe and Horne argued persuasively that we will not construct a human psychology until we include verbal behavior and its interactions with other classes of behavior. Interestingly, Lowe and Horne argued for including self-reports in our research. They noted that despite our reluctance to include self-reports, human beings do describe contingencies, and these descriptions do affect subsequent performance. The empirical evidence is clear, as indicated in this chapter and in other work by Lowe and his colleagues.

Variation and Selection

The chapters by Staddon (Chapter 6) and Richelle (Chapter 1) contained comment about the place of variation and selection in psychology. Richelle noted that we have neglected behavioral variation. Staddon suggested that this neglect reflects a historical emphasis on the elic-

iting function of stimuli when reflex integration and contingencies should have attracted the interest of psychologists. Added to that, Richelle noted that most work on behavioral variation has been theoretical rather than empirical (also see Smith, 1983). Probably more work on behavioral variation is available than this comment suggests. The literature on children's talking and spelling includes examples (e.g., Beers, Beers, & Grant, 1977; Ferguson, 1973; Foppa, 1978). Someone should review this material in relation to the operation of variation and selection in human behavioral development.

Basic Research and Behavior Modification

Several chapters contained material relevant to the debate about the relationship between basic research and behavior modification. Michael's chapter (Chapter 10) summarizes the issues and suggests what we might do to remedy the separateness of the two fields. In particular, Michael suggested that we need to return to Skinner's writings and build on them.

Lowe and Horne (Chapter 7) also contributed to the debate by raising strong doubts about attempts to apply the matching law directly to clinical settings. They showed that human subjects behaving under concurrent schedules formulate rules that affect their behavior and that may, but need not necessarily, produce matching. Lowe and Horne's chapter provides an argument for a strong return to basic, human research.

As well, Lowe and Horne noted the different purposes of applied and basic research. Applied work properly achieves practical results, whereas basic work properly isolates independent variables. This comment reminded me of Bunge's (1974) remark that we do not isolate independent variables and find laws on the battlefield, in the consulting office, and in other such settings. But the implied need to withdraw to the laboratory is easy to overlook because we are constantly pressured for immediate, practical results. This pressure is not new: Pratt (1939, pp. 175–176) deplored it decades ago.

Related to the demand for immediate results is the neglect of developmental issues, as noted by Lowe and Horne and by Richelle (Chapter 1). Much excellent work appeared in *The Journal of Experimental Child Psychology* in the 1960s and 1970s, but it quickly gave way to applied behavior analysis. In light of the new interest in a human operant psychology (e.g., Hake, 1982), this earlier work needs review so we do not miss its insights. Building on that earlier, short-lived tradition will require that we resist the demands for immediate application that led us into behavior modification. At least, we will have to resist these demands if we want to pursue science in addition to practice.

The debate about the relationship between basic research and behavior modification is important. It illuminates one difficulty facing us because we use behavioral control techniques as the instrumental base of our research. That difficulty is the pressure for immediate results and the subsequent dismissal of our entire enterprise by critics when the results of our efforts are trivial or transitory. We need further to clarify the relationships between basic research, applied research, and practice, and to explore their ramifications for our enterprise. It was good to see that this issue is alive and well in behavior analysis.

CONCLUDING REMARKS

Richelle suggested that European contributions might help us move toward an integrative synthesis that would go beyond the separation and divisiveness that plagues psychology. For that to happen, we need informed discussion about radical behaviorism or, rather, about a contingency-oriented psychology. Several chapters in this book present such a discussion, and they contribute importantly to our enterprise. But, some other chapters do not contribute to a contingency-oriented psychology, if only because their authors seemed unsympathetic to such a psychology or, perhaps, uninformed about it.

Consensus about our field *is* still emerging. But, allowing for that, some

chapters contained errors of scholarship that most of us would indeed count as errors. I have already mentioned that one author did not distinguish between radical and methodological behaviorism. Added to that, another criticized our enterprise for espousing a mechanistic causality. This misconception overlooks the central place given to variation and selection in a contingency-oriented psychology and our acceptance of action-at-a-distance, among other things. As another example, one contributor asserted that operant psychology takes movement as its dependent variable. Isolated statements by Skinner confirm this, but contradicting it are Skinner's rejection of the formalistic fallacy and his advocacy of an autonomous science of psychology, among other things. While the phenomena of interest to us depend on movements of the body, our most basic dependent variables are the members of a functional class. To give yet another example, one author described operant psychology as a special case of conditioning theory. This example is more contentious since the term "operant conditioning," still commonly used by us, encourages the notion that our interest is in conditioning. But "conditioning" is a word we could do without (Goldiamond, 1975); at the very least, its connotations conceal the nature of a contingency-oriented psychology. As a final example, one author characterized behavior analysis as a stimulus-response psychology. This latter imputation contrasted with Staddon's (Chapter 6) informed discussion about the distractions imposed on psychological thinking by the stimulus-response framework. We still need to abandon fully such stimulus-response thinking, for on an explicit level we plainly reject it.

Misconceptions are tiresome when found outside behavior analysis, but they seem inexcusable in a book entitled *Behaviour Analysis and Contemporary Psychology*. Perhaps the difficulties of presenting conference proceedings in an edited book make such inadequacies an unfortunate reality. Adding to this difficulty, the body of knowledge we have accumulated collectively is not readily

accessible, as Michael (Chapter 10) noted. To make matters worse, the insights of radical behaviorism are conceptually difficult, they depart fundamentally from tradition, and are themselves under revision and expansion. We need to find ways to communicate better the advances we have made to other psychologists and to investigators from other disciplines. This book might indicate our intention to communicate and to seek integration. My reservation is that parts of it suggest communication and integration at the expense of a contingency-oriented psychology.

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